buildings from the destructive effects of lightning. The controversy originated in some lectures on lightning conductors, delivered by Prof. Oliver Lodge at the Society of Arts in 1888; it was continued at the Bath meeting of the British Association, and it culminated in a paper, also by Dr. Lodge, read last year at the Institution of Electrical Engineers, in which, after stating that "the old views on the subject of electrical conduction are hopelessly and absurdly and dangerously inadequate," the author expressed the opinion that it was "time that the prophets of the old superstition were slaughtered by the brook Kishon." In the animated discussion which followed, Dr. Lodge's views were ably opposed by Mr. Preece and others, and the question can hardly yet be considered as definitely settled. Time will not admit of an adequate review of the arguments which were employed on the two sides, but, considering its great practical importance, I think it will be of interest to give a very short statement of the matter in dispute, which I will illustrate by copies of Dr. Lodge's diagrams and apparatus.

Ever since the time of Franklin it has been customary to make use of long pointed metallic rods for the purpose of protecting important buildings from damage by lightning; and the "older electricians," as Dr. Lodge calls them, have always taught that, if the rod were well made, of sufficient size and height, and properly connected to earth, it afforded practically perfect security over a certain limited area. The function of the rod was supposed to be not so much to receive the shock of a lightning flash as to prevent a flash from occurring at all in the neighbourhood of the protected building: this it did by promoting the silent discharge of electricity between the cloud and the earth through the point of the rod.

The lower of these two tinfoil-covered boards represents the earth, and the upper one a cloud; the upright metal rod with a ball at the top of it is supposed to be a church, or other building, erected upon the earth. Charging the apparatus by means of the electrical machine, we get a series of strong flashes between the cloud and the church, every one of which might do terrible damage. If now we place near the church another rod, with a needle-point at its end, to serve as a lightning-conductor, the flashes at once cease: however vigorously we work the machine, there is no longer any visible effect. The fact is, that the electricity is silently and harmlessly discharged as quickly as it is generated. In such a case as is at present represented by the model, the efficacy of a lightning conductor would be complete. This is what Dr. Lodge calls the case of "steady strain," and is that indicated in his first diagram [exhibited], where the charged cloud above the church spire is supposed to have moved into its present threatening position from a distance. According to Dr. Lodge, this is the only kind of lightning discharge which was ever contemplated by the older electricians.

But suppose that a harmless uncharged cloud which might be hovering over the church were suddenly to receive an overflowing charge of electricity by a flash from another more distant cloud. There would then be no time for any gradual relief of the strain by a silent discharge through the lightning conductor, and either the conductor itself or the church would infallibly be struck by a flash from the overflowing cloud.

By altering the connections between the model and the electrical machine,[1] we can easily imitate this condition of things. The tinfoil-covered boards now remain absolutely uncharged until the moment when there is a spark between the terminals of the machine: then they are suddenly charged, and a flash instantly passes between the cloud and the church. Placing the needle-pointed lightning conductor beside the church, we now find that it is powerless to prevent the flashes: they go on just as rapidly as before, striking either the conductor or the church, or sometimes both at once. This case, which, I think, Dr. Lodge was undoubtedly the first to call attention to explicitly in connection with thunderstorms, is called by him the case of "impulsive rush." The occurrence of an "impulsive-rush" flash, then, cannot be warded off by a lightning conductor. The most that a conductor can do is to divert the main shock of the discharge from the building to itself. But even so the lightning may do considerable damage, for, as Dr. Lodge says, "it is hopeless to pretend to be able to make the lightning conductor so much the easiest path that all others are protected. All possible paths will share the discharge between them, and lots of apparently impossible ones." Moreover, not only is the lightning conductor itself, when struck, liable to spit off sparks laterally, however good its earth connection may be, but other metallic bodies in the neighbourhood may do the same, whether such bodies are insulated or not.

The moral appears to be this. In all cases of steady strain in which a charged cloud descending from the upper regions of the air, or approaching from a distance, might inflict serious injury upon an unprotected building, a well-designed and properly earth-connected lightning-rod is an absolute safeguard. In a case of "impulsive rush," the rod may often be of use in bearing the brunt of the discharge, though sometimes the lightning will take no notice whatever of it, striking the building and altogether neglecting the rod; and it is even possible that a high rod might attract a destructive discharge which otherwise would not have occurred at all. Although, therefore, a lightning-rod is in many cases, probably in a very large majority, of the greatest service, it cannot be depended upon as affording perfect immunity from risk; and the assumption which has universally been made by the "older electricians," that damage by lightning is in itself conclusive evidence of some imperfection in the conductor, is an unfounded one.

In conclusion, it may not be out of place to say a word or two on the subject of personal danger from lightning. The spectacle of a severe thunderstorm, magnificent as it is, is no doubt calculated to inspire a certain amount of alarm. But statistics show clearly enough that, at least in this country, its bark is worse than its bite. It appears, from a paper published last year by Inspector-General Lawson, that the number of deaths caused by lightning in England and Wales from 1852 to 1880, as recorded in the returns of the Registrar-General, were 546, or rather less than 19 per annum. The average population during that period may be taken as 22 millions; it follows, therefore, that the average annual death-rate from lightning was considerably below 1 per million of the population. The risk of a fatal lightning stroke in any individual case is therefore exceedingly small.

SPORTS.[1]

IT is highly desirable that we should attach a definite signification to this word. Among gardeners it may mean many things, whilst, among botanists, it is restricted to cases of bud-variation as distinguished from variation from seed. In this note we shall use the word in its botanical sense, as applying to a special illustration of that tendency to vary which is common to all living beings. We shall, however, gain a clearer idea of what true sports are by the elimination of certain things which are not sports, though often called so. In the first place they are not seedling variations. Out of a hundred seeds of Lawson's Cypress that are sown it is possible, I suppose, to get ten more or less distinct varieties, besides others which are more or less indistinct. The great variability of this species is now well known, and the seedlings of *Abies subalpina*, Engelmann (*A. lasiocarpa* of Hooker), furnish another illustration of the same tendency. These seedlings may be the result of cross-fertilization between varieties, or they may be reversions to an earlier condition; at any rate, of whatever nature they are, they are not "sports" in the sense here intended.

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[1] The tinfoil-covered boards were connected with the outer coatings of the Leyden jars, their inner coatings being in connection with the terminals of the machine.

[1] Reprinted from *Garden and Forest*. The article contains the substance of an unwritten address lately given to a society of gardeners.

Next, sports are not mere stages of growth. Most plants put on a different appearance at various periods or stages of their growth, and sometimes these changes are very remarkable. The Retinosporas of our gardens furnish us with excellent illustrations. *Retinospora* (or more strictly *Thuya*) *pisifera* exhibits during its growth very different appearances in its foliage. There is the squarrose form and the plumose form, the golden form, the silver form, the pendulous form, the thread-like form, the upright form, and perhaps others. All these, however, are not separate entities; they may all occur on the same bush. If cuttings or if grafts be taken from the sporting branches they may be reproduced almost indefinitely.

Barring the mere colour variation, these forms are but stages in the growth of the plant, occurring with more or less regularity and in greater or less degree of prominence in all the individuals of the species, as may be inferred from watching the growth of seedlings in a seed-bed.

Other illustrations of variations arising during growth are afforded by the differences often observable in the foliage on the flowering branches as contrasted with that on those branches which bear no flowers. The common Ivy furnishes an illustration. The short contracted shoots of the Laburnum, or the Apple, known as "fruit spurs," constitute other examples.

Another form of variation in flowers is that connected with difference of sex. A "pin-eyed" Primrose does not greatly differ in appearance from a "thrum-eyed" one, yet the difference between them is precisely of the same character as that between the variously formed flowers of some species of Catasetum and Mormodes. So utterly different are the male and female flowers of some of these species that they were at first placed by very competent botanists in different genera. It was only when the Protean plants produced all the forms of flowers on one and the same spike, that it was seen that, so far from belonging to different genera, they did not even belong to different species. It was left to Darwin to show what this paradoxical variation really means; and now, when we meet with a case of the kind, we say, "Ah! yes; only a sexual form," just as if we had known all about it from our earliest years, and very possibly, in our haste, mixing up, or, at least, not discriminating cases of a different nature. But this is not what we propose to discuss just now; we simply say that these cases, though often so designated, are not sports, at least in our acceptation of the term.

What, then, are sports? We have already characterized them as "bud-variations," but we must give some further indication of their peculiarities: First, as to the suddenness of their production. A tree or a shrub, all on a sudden and without any cause that is apparent to the eye, will put forth a bud, which, as it lengthens into a shoot, displays leaves of a different character from any that the plant has hitherto produced, which have no definite relation to any particular stage of growth; and which are quite different from any that under ordinary circumstances the plant in question has produced or is likely to produce in future. In short, the occurrence is sudden and unforeseen. Gardeners, of course, avail themselves of these variations. They remove them, bud them, graft them, strike them from cuttings, or, in some way or another, endeavour to perpetuate the variety, and thus have originated many of our cut-leaved Beeches, Maples, and Limes. Thus, too, may have originated some of our weeping trees and some of our pyramidal shrubs, though, for the most part, these have, as I believe, originated as seedling variations.

Not only do these variations occur suddenly, but they are very local in their manifestation. One particular shoot "sports," while all the rest remain in their normal condition. It is very different in the case of seedling varieties, where the whole system of branches and leaves is more or less affected.

Another and a most remarkable feature about these sports is, that they sometimes occur simultaneously in widely different localities; thus the same sport of a Chrysanthemum "turns up" about the same time, not only in different nurseries in this country, but also on the Continent. This may be because all the plants in question have originated from one and the same stock.

These, then, are the special characteristics of a true sport. Illustrations could be given by the hundred; but neither time nor space permit, nor, indeed, for our present purpose, is it requisite to do so. Whoever will investigate the cause of these sudden outbursts of local variation must, of course, sedulously examine each case for himself according to the measure of his ability and of his opportunity. The circumstances, the history, the progress, the anatomy of each particular sport must be investigated, both absolutely and in relation to similar outgrowths in other plants. Until this is done—and it has not been done yet—any explanation as to the cause of the phenomenon must be a matter of speculation. Still, we cannot help guessing, and though we may be wrong in our surmises, at least the process does good by setting us observing and thinking. Observing and thinking are processes valuable to all of us, but in a particular degree to those who practice the cultural arts. And so it happens—or, at least, we will hope so—that although the causes which have been assigned for these changes are various, some, perhaps, utterly wrong, others partially so, and all more or less inadequate to explain the whole of the phenomena, yet some advantage may accrue from the discussion. An indirect benefit is better than none at all, and anything which enforces us to take some measure of the extent of our own ignorance is likely to be beneficial. We should never be a bit the better if we simply acknowledged our ignorance, as, indeed, we needs must do in any case, but directly we attempt to find out in what particulars and in what degree we are ignorant, then there is some hope that some portion of our "nescience" may be dispelled. Under this impression we may allude to one or two of the assigned causes of sporting. External causes are those which the gardener most generally invokes. For him a sport is usually the consequence of some alteration in the nutrition of the plant. It gets too much or too little food, or the food is not of a suitable character—containing too much of one thing, too little of another, or the climate is charged with the results observed. It is very convenient to have the weather to blame; it may be too hot or too cold, too moist or too dry, too brilliant or too obscure; or the soil may be at fault, the drainage may be defective, the earth not sufficiently aërated, its temperature too high or too low. Combined action of some of these conditions is, of course, possible, intermittent action equally so, whilst we, in this country, are abundantly familiar, first with one thing in the way of the weather, and immediately afterward with another. It is, therefore, not surprising if some gardeners, without troubling themselves much to see how the explanation fits the facts, do attribute "sports" to such causes as we have mentioned. To our thinking, the objections to this kind of explanation are fatal. External circumstances are, no doubt, potent enough to effect very great changes indeed. We are daily witnesses of them; but they do not produce the kind of change which we know as "sports." On the contrary, sports occur sometimes when no alteration of external conditions is perceptible, and they do not occur when such alterations are very apparent. Or, again, they appear in one place under one set of circumstances, and at another place, simultaneously, under a different state of affairs; and although all the plants growing together have been exposed to the changed conditions of life, the sporting tendency shows itself in one particular plant only, and in one particular part of that plant, generally only in one bud. With all respect, then, for those who hold these views—and one at least of our most experienced and eminent plant-growers has lately publicly advocated them—we venture to think external causes, however adequate they may be in some cases, are inoperative in such cases as we are considering.

A better explanation is that offered by Darwin, by Naudin and others, according to which sports are due to a dissociation of mixed elements, a reversion to the character possessed by one or other of the ancestors of the plant, perhaps one or two, perhaps an indefinite number of generations ago. Let us recall for a moment what a very composite thing a plant is, even such a one as we call a simple plant. At first it is neutral and homogeneous, a mass of protoplasm, but the homogeneity of protoplasm is a thing of the past. We do not believe in it now. On the contrary, we believe in frameworks and interstitial fluid, in granules and fibres, in some parts that are alive, others that are dead; some that are stable and immutable, others that are mobile and changeable; in short, we have come to the conclusion that, physically and mechanically, as it was previously known to be chemically, protoplasm is very much "mixed."

Again, another of our old beliefs has been dissipated. Once we were taught that the cells of plants were closed bags without apertures, and that, while the fluid passed from cell to cell by osmosis, there were no visible pores, and no means of transmitting anything more solid than cell-sap. The passage of protoplasm from cell to cell was not then thought of as possible. But Mr. Walter Gardiner has changed all that. He and others who have followed in his steps have taught us how to see the pores in the cell-walls, how to see the passage of protoplasm through those pores from cell to cell, and how complacently to employ the phrase "continuity of protoplasm" in a manner that gives us, at present at least, great satisfaction. These modern discoveries of the composite nature of protoplasm, and of its passage, at certain times and under certain conditions, from cell to cell, seem to us to furnish a clue to the explanation of some of these cases of sporting, as they do also in the case of some of those curious cases in which the stock seems to influence the scion, or the scion the stock, in cases of grafting.

Again, in the life-history of a plant there are several stages. There is the neutral stage, when it is, at any rate, so far as sex is concerned, an epicene. Then there is the sperm stage, when our plant consists of a mass of neutral matter, a particular portion of which is developed into sperm-cells, or into what will ultimately produce them. At another time the neutral cells of one portion of the general plant-mass develop into germ or female cells, or it may happen that both sperm and germ cells may be developed at one and the same time, when the plant has, of course, a three-fold constitution.

All these modifications occur in the course of the life of each individual plant. But each individual plant is, necessarily, compounded of elements derived from its two parents, so that, for illustration sake, if we may consider the original stock to consist of three portions—neutral, male, and female, respectively—it is obvious that in the first generation there would be six component elements; in the second, twelve; in the third, twenty-four, and so on. Who can count the generations of plants? It is enough for our purpose if we succeed in showing clearly the composite nature of plants

This being granted, it will not seem remarkable that occasionally a partial separation takes place, just as a scum may rise to the surface of some mixed fluid, or a sediment fall to the bottom of another. This illustration may, perhaps, serve to suggest the reason for the separation of mixed elements in plants; but that is too speculative a matter for us to enter upon here. It will be better for our present purpose to note one or two examples of dissociation of mixed characters wherein both the fact and its explanation are clear. One of the most interesting is that narrated by Mr. Noble, the originator of the white form of Jackman's Clematis. Noble's Clematis, as we may here shortly call it, is the result of a cross between Jackman's Clematis and *C. patens*. Soon after this Clematis was sent out, some dissatisfaction arose because, instead of producing flowers of good form and purity of colouring, more or less misshapen blooms of an unattractive appearance were formed. The matter was mysterious. The raiser was blamed by those who did not know that he is a highly competent man in his business, and one whose integrity is beyond question. The plant was condemned. Fortunately, however, the edict was not carried out in its entirety—some specimens were left. These were watched, and in due time afforded the explanation of the mystery. Jackman's Clematis flowers in the autumn on shoots formed during the spring and summer—on the new wood, as gardeners say, just as happens with a Rose. *Clematis patens* flowers in spring on shoots that were formed during the previous summer, on the "old wood," in gardening phrase. Now, when Noble's Clematis came to be scrutinized, it was found that it produced two kinds of flowers. Those which expand in spring are solitary, semi-double, never white, but bluish-gray, like those of *C. patens*. Those which unfold in autumn are produced in pairs and are single, like those of *C. Jackmanni*, but white. In the spring no flowers of the Jackman type are ever seen, and when the old wood is cut away, and only new wood thus suffered to produce flowers, no blooms of the *patens* character are seen, but only those of the Jackman type.

Another very interesting case of unmixing, or, if it be preferred, of partial mixture, is afforded by Neubert's Berberis. This is a hybrid between the evergreen pinnate-leaved Mahonia and the deciduous simple-leaved *Berberis vulgaris*, and it bears leaves some of which are intermediate in appearance, while others are like those of one or of the other of its parents.

The two illustrations above given are instances of the results of cross-fertilization, in which the whole process has, so to speak, taken place under our own eyes. But for how many centuries has the Chrysanthemum, we will say, been crossed and recrossed and crossed again? This process of crossing seems destined to come to an end, because the flowers, after a time, become sterile, owing to the fact that the stamens and pistils, one or both, are imperfectly or not at all developed. Seedling variations in such cases must become more and more rare as the process of sterilization becomes more and more marked. If new seedlings are desired, raisers will have to go back to less highly modified flowers—to flowers, that is, which are more nearly in their original condition. But although the production of varieties in the Chrysanthemum by fertilization is thus limited, the development of sports by bud-variation may, and probably will, still go on, to the delight of the grower and the interest of the student. It must, however, be said that at least in the case of the Chrysanthemum the change is sometimes very slight, depending solely on the presence of colouring matter in some cases and on its absence in others. The form of the flower and of the foliage in many of these Chrysanthemum sports is in no wise different from that of the parent plant. This is only an illustration of the fact that all degrees of combination or of dissociation, as the case may be, may be expected to occur.

Is there any commingling of the elements of stock and of scion in the case of grafts? Botanists and gardeners, almost without exception, have asserted that there is none. Place on a sheet of wet blotting-paper, which may represent the stock, a drier piece of the same substance, which may represent the graft, and there will be a passage of the fluid from the lower to the upper paper, but there will be no mixture of the constituents of the two.

We have always wondered, if there were no reciprocal influence of stock on scion, why grafting is practiced at all, because we cannot understand the acknowledged advantages of the practice except upon the supposition of some modification being exerted. Gardeners triumphantly, as they were quite justified in doing, pointed to the millions upon millions of cases where no such modifications are visible. Botanists pointed to the closed cells from whose cavities only the thinnest of liquids could exude and permeate through the walls of adjoining cells. This was before the days of "continuity of protoplasm," as above mentioned. Now that we know that not only water, but protoplasm itself, may, under certain circumstances, pass from cell to cell, the difficulties in the way of conceiving that any influence could be exerted on the scion by the stock, or *vice versâ*, are very materially lessened, if not entirely removed.

But before the time we speak of, there were some alleged facts which, provided the history given were true, could only be explained on the supposition of the commingling of elements by grafting and subsequent separation. In other words, the possibility of graft-hybridization must be assumed. Whether it has been proved is another matter.

One of the strongest cases in its favour that we know of is that of the famous Adams's Laburnum (*Cytisus Adami*). We cannot go into detail as to the history of this extraordinary tree. It must suffice to say, that it is stated to have originated from the implantation of a bud of the dwarf, shrubby, lilac-flowered *Cytisus purpureus* on to the common Laburnum. Be this as it may, we have in our gardens on this side of the Atlantic trees which every year astonish the beholder by producing, together with the foliage and flowers of the Laburnum, tufts of *Cytisus purpureus* and all sorts of intermediate conditions between the two. If the stock exerted no influence on the scion, the buds should be pure *Cytisus purpureus* and pure *C. Laburnum*, without any intermediate forms. It would lead me too far to give other illustrations of the production of shoots of an intermediate character between stock and scion. Many such are on record, and many have come under my own notice. It must suffice for me to show that whilst we may, with a very great amount of probability, attribute the existence of some sports to the "un-mixing" of elements blended by means of cross-fertilization, whether between species (hybrids) or between varieties (cross-breds), we may, likewise, but with a less degree of probability, attribute the existence of others to a similar dissociation in the case of grafted plants.

Obviously the latter cases must be much less numerous than the former, and are purely artificial productions, not likely to occur in Nature.

Other assigned causes appear to me to pertain rather to variation in general than to that limited, localized form of it which is here considered as bud-variation, and may be here passed with the mere mention.

Maxwell T. Masters.

A NEW SCIENTIFIC SERIAL.[1]

The imposing series of four octavo volumes before us is the embodiment of the first five years' work in the new Museum of the Austrian capital. Of the nature and plan of the building itself our readers have already been made aware; the collections housed within it are rich in types and specimens of priceless value, and its affairs are administered by a large and efficient staff of specialists, many of whom have attained a world-wide reputation. The directorship lies in the hands of Dr. Franz Ritter von Hauer.

Each of these volumes consists of four parts, and embraces one year's work. The parts are issued quarterly, their limitation in size being determined by the progress of work in hand. The first part of the first volume, issued early in the year 1886, is exclusively a "Jahresbericht" for the preceding year. It has already received notice in our pages (Nature, vol. xxxiii. p. 424). While for the most part a report of work done, it contains information concerning the Museum itself, together with a list of names of the officers and staff, and of the various donors, correspondents, and persons who studied in the Museum during the year, as of those to whom material had been lent, together with references to published works in the production of which the resources of the Museum had been utilized. Of the remaining fifteen parts, each contains one or more special treatises, together with "notices" of a miscellaneous character, correspondence, personalia, and administrative detail, with acknowledgments of acquisitions. The four volumes make up a total of over 1900 pages of closely printed matter, with 80 plates and numerous woodcuts. The illustrations are, for the most part, highly satisfactory; we would, however, have preferred the substitution of ordinary lithographs for the photographs of Ophiurids described in vol. ii.; the latter are too indefinite and unsatisfactory. Excluding the notices and miscellanea, which monopolize collectively 22 per cent. (415 pp.) of the printed sheets, there remain 1532 pages of a more solid nature, which make up the bulk of the collective volumes. These bear, in all, 55 treatises; some of them, as our pages have already borne testimony (Nature, vol. xxxv. p. 204), are lists of types and specimens in the Museum, others are elaborate monographs dealing with highly involved structural detail. The Museum is divided into five departments, each having its own working staff, and the published works bear the following ratio: zoology, 23; mineralogy with petrography, 13; geology with palæontology, 9; botany, 7; anthropology and ethnology, 3. As might be expected from this list, many new species of organic beings have been described. We find much to admire in some of the monographs; and especial attention is demanded by those devoted to the ethnology of the South Sea Islanders, by Dr. Otto Finsch, and to the artistic products of the Dyaks, by Prof. Alois Raimond Hein. These memoirs extend over the greater portion (240 pp.) of an average volume, and they are amply illustrated; the information contained in them is of inestimable value, the illustrations are of rare merit, and it would be difficult indeed to surpass the coloured representations of Papuan handiwork which adorn the pages of Dr. Finsch's important communication. These monographs are based upon the collections in the Vienna Museum, and upon perusal of them we know not upon which of their acquisitions most to congratulate our Austrian *confrères*—those of types of Nature's productions, or those of objects of human artifice. Moreover, the appearance of the memoirs cited, now that the South Sea Islanders are receiving renewed attention, is most timely; and their value is greatly increased by the fact that the peoples to whom they relate are becoming demoralized and demolished by the advance of "civilization."

The Museum whence these *Annalen* emanate was opened to the public in August 1889 by "His Apostolic Majesty the Emperor"; and an account of the ceremony, with its attendant honours, is to be found in vol. iv. The pages of the journal show the custodians of the institution to be fully alive to the value of their charge. The

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[1] "Annalen der k.k. Naturhistorischen Hofmuseums, Wien." Bd. I.–IV 1886–89. (Wien, Alf. Hölder.)